für das menschliche Auge in ewige Nacht gehüllten Strahlenbereiche entquellen, gar kein Ende nehmen. Mit jeder folgenden Region, die man zur Aufnahme einstellt, meint man das Endgebiet der kleinsten Lichtwellen zu erreichen. Aber es ist fast, als flöhe die kleinste Welle, die überhaupt noch photographisch zu fesseln ist, um so behender ins fernste Ultraviolett hinaus, je näher ihr die Fessel der lichtempfindlichen Platte rückt.

Schon jetzt weist meine neue Platte jenseits 1852 ein Spectrumband auf, dass das gesammte Wirkungsgebiet der Bromsilbergelatine um mehr als das dreifache an Länge übertrifft, und gleichwohl lässt auch die letzte meiner Aufnahmen noch der Hoffnung Raum, dass jenseits des Randes ihrer Platte noch photographisch wirksames Licht existirt. Vorläufig gehört aber diese letzte Aufnahme, ohngeachtet solch' günstiger Aussicht, doch demjenigen Gebiete an, das ich gegenwärtig als die Grenze der kleinsten Lichtwellen bezeichnen muss. Die Photographie des Nachbargebietes hiervon stösst zur Zeit,— aus Grunden, deren Erörterung hier zu weit führen würde, — auf Hindernisse, die sich, sofern es überhaupt möglich ist, nicht ohne grosse Anstrengung werden beseitigen lassen.

Fragt man nun nach dem Masse der kleinsten Lichtwelle meiner Ultraviolettaufnahmen, dann muss ich leider bekennen, dass mir im Augenblick eine bestimmte Antwort hierauf nicht möglich ist. Wellenlängen lassen sich im luftleeren Raume, an den meine Aufnahmen gebunden sind, nicht so leicht ermitteln wie in der Luft, und die geplanten Messungen der Wellenlängen des äussersten Ultraviolett haben darum auch besonderer Vorbereitungen bedurft. War es doch überhaupt zweifelhaft, ob sich die übliche Methode der Messung der Wellenlängen auf den in Rede stehenden Lichtbereich werde anwenden lassen. Meine Vorversuche hierzu gehen zur Zeit ihrem Abschluss entgegen, und die mir vorliegenden Resultate berechtigen zu den besten Hoffnungen. Unter solchen Umständen kann ich das Mass der kleinsten Lichtwelle, die meine Aufnahmen aufweisen, vorläufig nur schätzungsweise und mit Vorbehalt nennen. Es dürfte dieses Mass 1000 AE nicht überschreiten, ja eher kann es um ein gutes Theil kürzer sein.

Der Wellenlänge 1000 entspricht eine ausserordentlich hohe Schwingungszahl des Lichtäthers. Während die brechbarsten Sonnenstrahlen wenig mehr als 1000 Billionen Schwingungen in der Secunde ausführen, schwingt ein Strahl von der Wellenlänge 1000 in derselben Zeit dreibillionenmal. Mit Schwingungszahlen so enormer Höhe hat der Spectroskopiker bisher noch nie zu rechnen gehabt, und gleichwohl liegt es nicht ausser dem Bereich der Möglichkeit, dass wir über kurz oder lang die Wirkungen des Lichtaethers bis in die nächste Nähe der Wellenlänge Null verfolgen werden, wo der ungeheuren Anzahl seiner Schwingungen kaum noch der Massstab des Endlichen gewachsen ist.

THE SOUNDS OF R.[1]

BY ALEX. MELVILLE BELL.

THERE seems to be special need for a better understanding of the sounds of R. No element of speech is so variously pronounced — in dialects and by individuals. The fundamental organic action from which all the varieties are derived is a frictional emission of breath or of voice between two surfaces in the breath channel. Thus we may make an R in the throat,— producing the effect which, when prolonged, is called a groan; or in the guttural passage, between the back of the tongue and the soft palate — a mode which is dialectically common in many countries. A less definite variety is formed between the arched top of the tongue and the roof of the mouth. This is common in the United States. Another — and the normal English form of R — is produced between the point of the tongue and the upper gum. This is sometimes modified by inversion of the tongue within the palatal arch, or by addition of guttural or of labial contraction. The point-tongue R is also varied by advancement of the tongue to or between the teeth. In a common English affectation the seat of R is transferred fron the tongue to the lips, so that R has the sound of W. Of these varieties one may be characteristic of a dialect, another a mere individuality, but they are all effects of only one organic action performed at different parts of the mouth.

Another series of R's results from a trilling or rattling organic vibration instead of a mere friction of the breath or voice. Thus a trill of the epiglottis is heard as one form of R; a trill of the uvula is another and very common one; and a trill of the point of the tongue is the regular form of R in North Britain and Ireland. The Spanish R has a more prolonged rattle of the same kind. The trill has often the effect of a syllable; as in Scotch and Irish, where it converts the grammatical monosyllables *world, harm, mourn*, etc., into the phonetic dissyllables wor-rld, har-rm, etc.

These trills involve a strong pressure of breath and a harshness of phonetic effect, in contrast to which is a form of R of simple vowel quality, without friction or vibration; as in (ə)ise and (ə)ound, for *rise* and *round*.

A similar vocalic effect is also heard for R wherever it is not followed by a vowel; as in *here, care, fire, store, tour, are, war, term, first*, etc. The syllable-like quality of this sound is distinctly felt after the close vowel ē, and less distinctly after open vowels, because their mouth-cavities differ so little from that of R.

In Early English R was always trilled, as it continues to be in Scotland, where most of the characteristics of Early English are still prevalent. But in modern English the trills have been softened away wherever R follows a vowel, until little is left of the R but its vowel quality. We are accustomed to the entire omission of R in negro speech, where *do* and *sto* are all that we hear for *door* and *store;* but in educated utterance there is some phonetic effect left in R even where it is least manifest. Such delicate shades of sound are the distinguishing marks of refinement in pronunciation, and they should be carefully preserved by teachers and by writers on phonetics.

In a book recently published in England the learner is taught that R is silent in such words as *farm, serve, lord, prayer, weird*, etc. Had the statement been that the sound of consonant-R is not heard in these words it would have been correct, but the R is certainly not "silent;" it has a phonetic effect of its own, soft and vowel-like, but a quality wanting which the words would not have their characteristic pronunciation.

That there may be no mistake as to the teaching in the work referred to, the reader is specifically told that the words *arms* and *lord* are exactly the same to the ear as the words *alms* and *laud.* Now what is the sound of R which baffles the discrimination of this writer? Let us magnify it, as in a microscope, by prolonging the elementary sounds. First let us put "alms" and "laud" under the microscope: —

a - - - - lms; lau - - - - d.

Here there is no R; the vowel remains unchanged until stopped sharply by the succeeding consonant. Now put "arms" and "lord" under the microscope : —

a - - - - (ə)rms; lo - - - - (ə)rd.

Here between the vowel and the m or d there is interposed a gliding connective sound, so that the vowel is not stopped sharply by the consonant, but its quality is gradually changed by a lift of the tongue, verging towards but not quite reaching the position for R. This is all the sound that R has, in modern English, before any consonant or when final in a word. But it is something more than n o t h i n g; and something that is essential to the correct utterance of any word containing R before a consonant.

Among the sounds of R may be reckoned the influence of R upon other sounds. The mouth-cavity for R being very large, any closer vowel preceding R is, as it were, s t r e t c h e d at the point of junction, so as to assimilate with R. Thus a pure ē is with difficulty pronounced before R; a pure ā is never, in Anglican speech, heard before R, but a is "stretched" to eh, as in *air, chair.* So, too, o and oo before R have a more open than their usual formation, as in old — ore; pool — poor.

These widened sounds of o and oo are distinctly different from the sound of aw; yet in the book before referred to the words *shore* and *drawer* are said to have the same vowel; and the words

[1] Paper read before the Phonetic Section of the Modern Language Association, December, 1891.

your and *yore* are classed as identical in sound. Your, shore, and drawer are thus "phoneticised" into yawer, shawer, and drawer. These words are, indeed, often so pronounced in dialectic speech; but the science of phonetics must be retrograding instead of advancing when, in an "Introductory Science Text-Book," such differences can be ignored, and such mere negligences cited as examples of correct usage.

All short vowels stop sharply on consonant-R, as on other consonants, as in *parrot, very, spirit, sorry, hurry;* but long vowels take on the connective glide even before consonant-R, as in *weary, fairy, wiry, gory, fury.* Thus wea(ə)ry, fai(ə)ry, wi(ə)ry, etc.

The vowel quality inherent in the mouth-cavity of R is that of er-ir in *her, sir.* Consequently, in such words as *firm, yearn,* the r has the effect of lengthening the syllable by making it contain two sounds of the same vowel. Let us put the words under the microscope: —

fi - - - - (ə)rm; yea - - - - (ə)rn.

Test this further by analyzing the syllable "word." If the r were "silent," the vowel would be sharply stopped by the consonant d. Thus, wo - - - - d; but the true pronunciation of this syllable interposes a glide between the vowel and the d. Thus, wo - - - - (ə)rd.

In forming this smooth transitional r the tongue is slightly lifted from the bed of the jaw; therefore when a vowel follows the r, the consonant sound of the letter is also developed; as in fearing = fear ring There is a tendency among many speakers to finish all open vowels with this lift of the tongue, so that the consonant r is inadvertently interpolated between two words, as in "Pennsylvania-r-Avenue, "I saw-r-it all."

Nice distinctions — like those which are the subject of this paper — are of no importance where mere intelligibility is concerned; for example, in the speech of the deaf. In such cases, the widest differences may be disregarded, so long as the words are understood. But in the study of phonetics, the most minute varieties require to be distinguished, because what in one case may be a distinction with but little difference, may in another become a very shibboleth.

I make no apology for introducing so small a topic to your attention. In a practical subject nothing is too small to be carefully investigated. The whole organism of speech is but small, and the differences of organic action from which the greatest elementary distinctions result are, in actual measurement, exceedingly small.

The sounds of R, with all their differences — rough, smooth guttural, lingual, labial, definite, indefinite — are only one in kind; and we must recognize them in their faintest as well as in their most obtrusive forms.

ON THE SECULAR MOTION OF A FREE MAGNETIC NEEDLE.[1]

BY L. A. BAUER.

A MAGNETIC needle suspended so as to move freely in all directions will set itself tangent to the lines of terrestrial magnetic force. At any particular epoch it will have a definite direction. It wil make a definite angle with the meridian, which, measured in the horizontal plane, is known as its declination, also a definite angle with this plane, which, measured in the vertical plane, is termed its inclination or dip. About this mean position of equilibrium a variety of small periodic variations take place, accompanied at times by fitful or irregular ones, which occasionally become quite respectable. Concerning this we shall have nothing to say. But the needle undergoes another, and by far the largest excursion, requiring centuries for its fulfilment. Since its discovery in 1634 by Gellibrand, as exhibited in the secular variation of one of its co-ordinate angles, the declination, it has been the cause of no end of fruitless speculation. It has engaged some of the best minds and given rise to most ingenious theories, but the riddle is still unsolved.

As the needle assumes different positions for different epochs, it gradually sweeps out in space a cone, whose vertex is the centre of gravity of the needle. Or, if you describe a sphere having as a centre the centre of gravity of the needle, and prolong the axis of the needle until it intersects the sphere, the successive intersections will form some tortuous curve. The geometric nature of this cone, or of this tortuous curve, remains to be investigated. A preliminary analytical attempt was made by Quetelet in 1877. He used fifty years of continuous observations of declinations and dip made at Brussels, and found that a cone of revolution would best fit his observations, the period of a complete revolution being 512 years.[2] Mr. Schott made a graphical attempt for an average New England station, using about fifty years of observation. The scantiness of his material prevented him from making any safe deduction as to the course of the needle.[3]

To our knowledge, however, no attempt has as yet been made for the long series of observation which we possess at quite a number of stations The usual custom is to discuss *separately* the secular variation of the different magnetic elements, as though they were *different* effects of forces acting, instead of *component* effects. We believe that this, in some measure, is the reason that with 100–300 years of observation no greater headway has been made in the conception of the requirements of the secular-variation problem.

With the view presented of the problem, some of the interesting questions we may ask ourselves are: Will the orbit described by the north end, say, of the needle, be a closed curve or approximately so? That is, will the needle at the end of a certain period assume the same direction that it had before, and again sweep over the same curve in the same length of time? Or, will the needle never return to a previous position, and thus never fulfil a secular variation period? If we have such a thing as a true period, is it the same all over the globe? If we have to deal with *different* periods, as the discussions of declination observations at various stations would seem to indicate, are these local or independent, and thus belong to different systems of magnetic forces? Or, do they but indicate different *stages* in the development of the secular variation, whereby either the period itself is a fluctuting one, or the orbit consists of several branches or loops? If the secular waves travel from east to west, traversing the whole globe, then by making an instantaneous circuit of the earth in an easterly direction, shall we find the needle at every station farther along in its secular orbit? Shall we find a continuously progressive and consistent motion throughout our survey, thus correlating the stations and referring the cause to a common origin? If we find this to be but roughly so, then by selecting as a base station, one where we have a long series of observations, we may with the aid of the shorter series at other stations, by adopting a time-coefficient determined from a comparison of the curves, attempt to answer some of the questions propounded without waiting until centuries have given us a complete period? Finally, what is the law of force acting upon the needle to cause it to describe its orbit?

To carry on a study of the secular variation to the best advantage, it would be highly desirable that at all stations where we have a tolerably long series of observations they be put in the best shape possible by one familiar with the subject and the station. It would then be an easy matter to establish secular variation stations all over the globe, where future observations might be made. This would mean simply the inauguration of a grand scheme, the fruits of which might not be seen for centuries. While such a gathering of material has already been made for many stations, there is, however, abundant material left.

The first station selected for discussion is London, where we have the best series of observations of both elements. The declinations date with Boroughs's in 1580. They can be represented (within their probable error) by the following formula, derived

[1] Abstract of a paper read before Section A of the A.A.A.S., Aug. 18, 1892.

[2] See Bulletins de l'académie royale de sciences etc. de Bruxelles, 47me année, 2me serie, T. xlv.

[3] See U. S. Coast and Geodetic Survey Report for 1885, App. No. 6, p. 272.